The spherical tokamak path to fusion power, revisited.

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Compact spherical tokamaks (ST) have been proposed by *Stambaugh et al,* [1], as a pilot plant for an ST power reactor; and by *Hender et al,* [2], as a powerful neutron source in this case with Q_{fus} (P_{fus}/P_{input}) approaching one. With new advances in ST physics and relevant technologies, we revisit these proposals on the ST path to fusion power.

Recent results from ST confinement studies [3] suggest an enhanced dependence on toroidal field (TF) relative to large aspect ratio tokamak scalings represented, for example, by the ITER ITB98(y2) scaling. The progress with the development of high-temperature superconductors (HTS) and demonstration of the first applications of HTS in tokamak magnets [4] opens the possibility of a realisable high-field ST. Such a device would make Stambaugh's high-field ST designs more economical due to the elimination of the resistive dissipation in the TF magnets. It would allow an increase in TF above 2.5 T, as used in the device proposed by Hender, and should lead to higher performance with Q_{fus} increasing to ~3 – 4. The use of HTS means that the space in the central stack can be used mainly for structural materials and shielding – the volume required by the HTS tape would be relatively small.

Several steps can be considered on this revisited ST path to fusion power. In addition to the on-going studies of the ST physics and development of HTS magnets, a high-field ST as a research facility would confirm predicted improvements in performance at high TF, leading to reducing risks in predicting the performance of the ST pilot plant. A steady-state ST compact neutron source [5] could also be considered as a steady-state device with relatively low, but still significant for technology development neutron production. The main aim of a pilot plant would be to demonstrate burning plasma (Q_{fus} > 5) under steady-state operation conditions. It would also be capable of demonstrating relevant technologies and performance for net electricity production that could be scaled up to a reactor. The pilot plant should use power plant relevant technologies to the maximum extent possible with available materials. To minimize the capital cost, the size of the device would be the minimum possible for achievement of burning plasma conditions. A high-field (~5T), highly elongated (k ~ 3.0), compact spherical tokamak with HTS magnets without a tritium breeding blanket can be proposed as the most promising candidate. Neither the amount of fusion power, nor the life-time of the device are the driving constraints of the design; however both targets should satisfy requirements for the future development of power plant technologies. The full power demonstration phase with the duration determined by the materials lifetime and other constraints connected with the high neutron load, will be preceded by low neutron flux D-D and short-pulse full neutron flux D-T operations with the objective to demonstrate $Q_{eng}^{DT}{}_{equivalent} > 1$ in D-D followed by ignition/burning plasma conditions in D-T pulses and then by $Q_{eng}^{equivalent} > 1$ explorations.

Because the ST is relatively small it will be relatively quick to build. In consequence it is likely to be the first ever burning plasma device, significantly advancing fusion research towards the goal of fusion power. The ST path exploits the advantages of a high field ST assuming that the plasma confinement scales strongly with the magnetic field, even in the low-collisional burning plasmas. Its physics performance cannot be fully predicted based on existing experience or on the experience gained by fusion research during the design and construction. Although our predictions are based on the best to date knowledge, there is always a possibility of "unknown" effects appearing in this new science of a burning plasma, self-sustained and self-heated by the fusion reactions. So its performance is difficult to predict with a credible confidence. But this uncertainty in the physics of the burning plasma is a risk in all paths to Fusion Power. The apparent advantage of the HTS based ST is that potentially a burning plasma can be realized relatively quickly and at relative modest cost. However, if the required performance for $Q_{eng}^{equivalent} > 1$ cannot be achieved, it is highly likely that increasing the magnetic field will increase Q_{eng}, probably at the cost of increased loads on materials (with a consequential reduction in the device lifetime). As a back-up, or as a next step, a second, probably bigger device could be considered for full power D-T operations, leaving the first device uncontaminated and available for further upgrades and modifications. This next step device could also be a first power plant demonstration module. As a first stage, the pilot plant and the first power plant module could be developed for pulse (several hours) operations for electricity production while the idling time could be used for low-power operations, annealing, maintenance or other purposes.

The pilot plant would not be designed to actually generate electricity commercially as this would be an unnecessary cost for a first pilot plant. However, it must demonstrate that usable energy could be extracted from the system with thermodynamic conditions that would permit conversion to electricity with available technology. Demonstration of the electricity production may be achievable in just one blanket module. Other blanket modules could be customised for other applications. Whether all technologies of the pilot plant could be directly transformed into a power plant module would depend on performance of the pilot.

A second risk on the ST path to Fusion power is connected with the desire to rely on fast progress in the development of materials, both for the first wall/divertor and for magnets. If such progress is slow, the life-time of the device may be reduced, and the price for magnets may be increased due to increased amount of HTS tape needed. However, at present, improvements in the performance of HTS are occurring rapidly both due to the progress in technology and increasing competition in the HTS production market.

A third risk is connected with the desire to make the design as simple as possible and minimize needs for maintenance and repairs. It is assumed that the main operational issues will be resolved during low-power (H and D-D) phase of operations, when some repairs will still be possible.

This approach to tackle the possible risks will allow demonstration of the pilot plant as a prototype for the first of a kind ST Fusion power plant module, Fig.1. Although

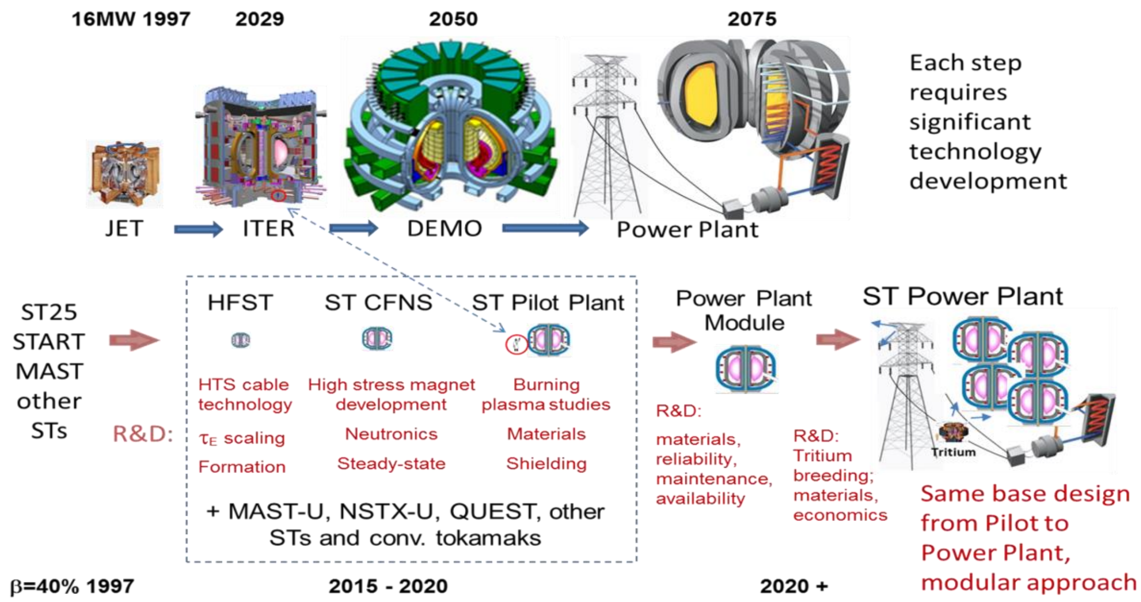

Fig.1. Comparison of conventional and ST paths to Fusion Power. The line between ST pilot plant and ITER is pointed at an average-size person to show the scale.

important contributions are expected from MAST and NSTX upgrades and from conventional aspect ratio tokamaks, as mentioned above, a high-field ST as a research facility and a steady-state ST Compact FNS are needed primarily to determine the energy confinement scaling at the high magnetic fields needed for pilot plants and reactors: hopefully the enhanced scaling seen at lower fields will be confirmed. On the step from the pilot plant to the first of a kind fusion power module, the main challenge will be in the development of materials that can allow significant (necessary) increase in the device lifetime. This issue will need to be addressed by a Component Test Facility based on the tokamak or another approach (e.g. IFMIF), as in the conventional fusion programme utilizing large-scale high aspect ratio devices.

The compact ST approach opens the possibility of constructing power plants based on "modules". The power plant would consist of a number of low-power but high-Q reactor modules which share infrastructure (i.e. start-up current drive systems, tritium plant, remote handling etc.). The conventional approach is to have the tritium breeding ratio (TBR) >1 but perhaps an alternative approach would be to run the power plant module with the TBR < 1, thereby simplifying the design and reducing the size of the module. In this case the tritium supply would need to be toped-up. In principle, this could be done with a nuclear fission reactor or a compact Fusion Neutron Source operating at the reactor site. Each module should operate with minimum maintenance and without the need for replacement of irradiated components until the end of its life, significantly increasing the availability and so reducing the cost of electricity. The modular approach allows reduction in the capital cost in next-of-a-kind devices as the single module is not expensive. Construction of a number of such modules may be possible within existing industrial supply chains. The prospect of a continuous "pipeline" of orders, and the financial affordability of such devices, could make this approach attractive to the private sector. In this scenario the low power and relatively inexpensive pilot plant could actually be very close to a first-of-a-kind power plant module. A commercial power plant may then be able to achieve economically optimal electricity output by multiplying the number of modules, rather than by increasing the reactor size or the output power. The time-scale to achieve this goal could be short relatively to that to achieve commercial fusion on the ITER/DEMO line.

The progress in application of HTS in tokamak magnets has been reported in [6], and construction of the first full-HTS tokamak ST25-HTS is on-going.

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